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NOTICE

Please come as early as you can to Iowa State University so you can attend the first Engineering Economy Committee Conference on Monday afternoon, June 25, at the Annual Meeting of the A.S.E.E. Our guest speaker from industry on that occasion will be:

Stephen T. Heinaman
General Supervisor
Corporate Budgeting and Accounting
Armstrong Cork Company
Lancaster, Pa.

His topic will be: "An Evaluation of Past Company Decisions in Equipment Replacement."

Our luncheon speaker will be:

W.W.Mc Callum
President
John Morrell & Company
Ottumwa, Iowa

A large attendance is important on these occasions.

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CAPITAL INVESTMENT DECISIONS- A GROWING CHALLENGE TO THE ENGINEERING ECONOMIST

by
Dale Zand
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Last year industry invested thirty billion dollars in new plant and equipment. Add to this already high annual investment the fact that for the past five years the rate of investment has increased beyond all expectations and you have a blazing signal of the growing challenge that faces the engineer. Capital investment decisions pose difficult problems for management, and in the past management has relied on engineers to find the answers. This is not so true any more. In fact, the situation is changing so radically that if the engineer doesn't quickly update his approach to investment decisions he will be left behind with some dusty capital recovery tables.

Usually, investment decisions are more of a nuisance than a challenge to engineers and product design attracts most engineering talent. In product design the engineer enjoys the fraternity of working with other engineers. He practices his technical skills without "resistance" or "interference" from management. Capital investment decisions, however, put management in close contact with the engineer on a more equal basis. The engineer is no longer on the firm, familiar ground of superior technical knowledge.

In capital investment decisions, technical facts, and assorted formulas may be helpful to management -- but more than anything else, they seem to confuse and delude the engineer. The engineer's formal preparation for making capital investment decisions consists of a course in engineering economy. Unlike the traditional fields of science, engineering economy has but a sparse body of research and technical literature, so that the engineer gets no more than the average management student — if anything, he gets much less.

As taught to the engineer, engineering economy is usually a one-semester course based on economics, cost accounting, simple mathematics, statistics, interest rate theory and a heavy dose of questionable depreciation theory. This is only part of the subject matter that the average management student absorbs in four years, yet the engineer is expected to master the field in one semester.

Because his knowledge of capital investment decisions is largely derived from management sciences, the engineer suddenly finds himself on the defensive. Management is just as informed as he is. This reversal of status is so strange to the average engineer that he is not aware of what has happened. If he is ever to recognize that his customary behavior is no longer appropriate, it is vital that the engineer develop a broader understanding of capital investment decisions.

In simple terms, a capital investment is a problem of determining how a company should invest its limited funds in new plant and equipment. Such decisions require many tenuous assumptions about the future and many difficult-to-justify choices in the present. Engineers and management agree on the need for assumptions and choices; but when it comes to making specific assumptions and choices, they are far apart.

To get a better picture of the problem, it is necessary to examine two areas of misunderstanding: (1) The characteristics of a capital investment decision, and (2) The entire process of investment decision making.

Starting with the first area, a capital investment decision has these three distinguishing characteristics:

1. It is a "genuine" decision.
2. It is a "high-penalty" decision.
3. It is a "long-period" decision.

Genuine Decisions

In genuine decisions, the decision maker re-examines and re-organizes his psychological field in terms of its scope, its depth, and its over-all configuration. In the presence of strong motivational forces, he questions and reconsiders all customary notions about values, goals, expectations, courses of action and standards.

The elements that distinguish a genuine decision from habitual behavior are flexibility and rearrangement. Here, flexibility means the ability to discover and evaluate alternative goals, expectations and methods. Lack of flexibility is termed rigidity, meaning the ability to respond only in a fixed manner because no alter-

native response has been conceived and integrated into the person's psychological field. This leads to the concept of rearrangement or restructuring. The result of considering and evaluating alternative goals, expectations and methods is a reorganization and reorientation of the psychological field. (1)

The engineer who has traditionally used Ohm's law to solve problems in current flow, goes through a psychological reorientation when he learns that Ohm's law is a special case that holds true only when certain restrictions are placed on Hertz's field equations. As the engineer encounters problems involving high frequency currents, he more and more sees the very limited scope of Ohm's law.

If the engineer cannot break through to the more sweeping concepts inherent in Hertz's equations, his limited psychological field necessarily limits him to the Ohm's law approach. This is rigid behavior. To be sure, it is a rather sophisticated rigidity, but in the design of electromagnetic radiation systems it is a fatal rigidity.

Habitual behavior, which means rigid behavior, is not absence of decision. It is simply unanalytical repetition of past decisions. Changes in environment go unnoticed. Whether this happens consciously or subconsciously, does not matter. The important unalterable fact is that the individual's psychological field is not restructured and the end result is habitual behavior.

Habitual behavior is found in every field of endeavor. Fortunately, it is satisfactory in most daily situations. For example, retailers find that for the most part a forty per cent mark-up is a satisfactory method for arriving at a selling price. Engineers find that for many simple replacement problems the habitual minimum annual cost approach is satisfactory. There are many equipment problems, however, that are neither simple nor replacement, and the minimum annual cost approach, or any other single approach for that matter, is completely inadequate. It is vital that engineers recognize that they are making the minimum annual cost calculation a symbol of behavior that is just as rigid as the forty per cent mark-up is in retailing.

High-Penalty Decisions

A capital investment decision is costly in many ways. First of all, it ties up a substantial amount of money. This money, in a sense, is frozen and cannot be shifted to another part of the business where it may subsequently produce more income, the familiar problem of sunk cost. Management's ability to adjust to unforseen conditions that require additional capital is reduced.

The cost of extricating the company from a poor investment decision is, for practical purposes, the amount of the original investment. Once the investment is made, even if the company should try to sell its equipment there is no ready market. Few investments, however, are so poor as to be completely unusable so disposal of equipment through sale is rare. Usually management uses the equipment as best it can. It often means in practice that this poor past decision becomes a millstone on future actions. Every effort will be made to "recover" the poor investment by continuing its possession until the book cost is written off.

Unsuitable new equipment becomes the standard excuse for not meeting production schedules, or for manufacturing at high cost. Gradually the new equipment is abandoned in favor of older equipment. An uninformed visitor to the plant will be struck by the paradox of old equipment operating at full capacity while new equipment stands idle. Such a condition, regardless of all unforseeable circumstances, is potentially an insurmountable blow to a decision maker's status.

For these reasons: (1) substantial outlay of funds, (2) decreased financial and technological maneuverability, (3) difficulty in disposing of unsuitable equipment, and (4) potential damage to personal status, compared to everyday decisions, a capital investment decision is a high-penalty decision.

Long-Period Decisions

It is well known that recovering the original capital invested in equipment takes several years. But more important, if any capital is to be recovered at all, the equipment must be used. The equipment must produce an economically valuable product. To most engineers this is a petty assumption. To management, it is an

annoying dilemma. This unavoidable necessity to forecast the demands of fickle consumers and to foresee the product changes of clever competitors is what makes management view a two-year period as an age and a five-year period as eternity.

Of course, there are companies that enjoy a quasi-monopoly position -- for example, utilities supplying electric energy. But in a society that demands more service at lower costs and with the increasing availability of petroleum, natural gas, atomic energy and solar energy even the supposedly favored utilities look to the future with increasing uncertainty. Now they too find that one year is a long period.

A capital investment decision is a decision to freeze the company's future production techniques in whole or part. When management acquires new equipment, even if it is to manufacture the right product, there is the ever-present threat of obsolescence - the threat that a competitor will devise equipment and methods to make the right product at substantially lower cost. This ominous and inevitable forward movement of competitive technology strikes with little warning leaving behind masses of obsolete equipment. Management wouldn't think that two years is a long time if engineers would develop a reliable index of functional obsolescence. (But, even the determination of obsolescence is itself a problem of decision making.)

Management tries to limit long periods of vulnerability in two ways. (1) It selects projects with a short period of capital recovery -- this is sophisticated language for the rule-of-thumb heresy of quick pay back. (2) It attempts to get government approval of rapid write-offs in order to bring the rate of book depreciation closer to what it believes is actual obsolescence.

In any event it would be well for engineers to realize that just as the value of the dollar varies and creates havoc with depreciation charges and replacement costs, so too all time periods do not have the same significance in capital

investment decisions. A two-year period say from 1930 to 1932 from a capital investment viewpoint is quite different from the two years between 1940 to 1942. And both of these "equal time periods" are drastically different when compared with the period 1950 to 1952.For investment decisions the conventional calendar approach to measuring time is as inadequate as the simple counting approach to measuring the value of dollars. One calendar year in a highly competitive industry facing rapid technological change is an eternity to a company without an established reputation to trade on and without a strong financial position to sustain the company through a year of deficits. This same calendar year is but a calm moment in the majestic life of a company with up-to-date equipment, with sound knowledge of likely advances in manufacturing processes and with a firm reputation in its markets. Time does not come in simple, distinct calendar periods when you make investment decisions. Time is compressed or expanded by the multitude of circumstances that make a company more or less vulnerable.

Product Design Decisions

Engineers are primarily trained in the art and science of making product design decisions and the bulk of their industrial experience is in making this type of decision. The critical question then is: How do product design decisions differ from capital investment decisions?

Strange as it may seem, the decision making process is the same in both cases. Goals must be established, assumptions about the future must be made, and alternative methods must be devised and evaluated. The problem is to recognize that the goals, assumptions, methods and values used in product design are not appropriate when they are applied without any changes to investment situations. It is like explaining to an excellent mason that even though he is a master in his craft he cannot follow the standards and techniques of bricklaying and expect to be an excellent sculptor.

In product development and design, materials and designs are studied from two ideally simple and appropriate engineering concepts: (1) function, which is simply efficient physical performance. The engineer first asks, "Will it do the job?". (2) economy, which is simply lowest dollar cost. The engineer next asks, "Which material or design will do the job at lowest cost?" The concepts of function and economy are basic to mastering any field whether a person is a writer, painter, engineer, economist or manager. Function and economy, however, vary in meaning in different fields of endeavor.

We must learn that by transferring the ideas of function and economy held in one field to a different field we are imposing values in a new situation where they are usually inappropriate. It is undesirable to take function as understood in engineering and expect that it should be applied without modification to art. It is undesirable to take function or economy as understood in government and expect that it should be applied without modification to business management. The reverse of these statements is also true. Function or economy in art and business are not properly applied to engineering or government.

"What constitutes economy in government?" Historically the answers to this question have been as numerous and as varied as the population of the world. Each of us tends to have a partial view of function and economy in government that is in the main a simple extension of the values practiced in our special field of endeavor. Rather than seek more information and attempt to enlarge one's psychological field, we find it easier to carry to other fields what we already practice -- to expand the scope of habitual decisions beyond their area of suitable application. In particular, the engineer has fastened on the idea that economy means lowest cost and that function means efficient physical performance. In product design and material selection he has found this approach very successful. There is great danger, however, in extending this approach to capital investment decisions. Especially since these decisions are much broader in scope and more severe in consequences than simple product design decisions.

Without realizing it the engineer has become the unwitting apostle of the great fiction called "economic man". By transferring values that prevail in product design decisions to capital investment decisions the engineer is naively publicizing the notion that in an economic situation, as if a purely economic situation could exist, man ignores human and social values and seeks only to maximize profit. Strangely, when called upon to make decisions in areas other than product design, engineers are keenly aware of human and social values. Thus it usually comes as a startling revelation to engineers that when they do not reexamine the values and attitudes of product design decisions but carry them over to other decision areas they are the greatest practitioners and perpetuators of the narrowest concept of economic profitability.

Unfortunately there are enough members of management who talk as if cost efficiency was the only goal of investment decisions to encourage engineering belief in this goal. Actually, what management says is quite different from what it does when it gets down to the practicalities of investment decisions. As a matter of fact, management's actions frequently contradict its alleged interest in cost efficiency. People outside of the management decision making council cite this contradictory behavior as evidence of managerial incompetence and unpredictability. Outsiders, however, are rarely in a position to appreciate that what appears to be capriciousness to them is in reality an attempt to make a genuine investment decision. Such a decision seeks to satisfy a spectrum of goals that is likely to include survival, liquidity, cost efficiency, maintenance of competitive position, continuity of sound labor relations and avoidance of an excessively high breakeven point. Too often, management is only vaguely aware of these other goals, that is - goals other than cost efficiency. These other goals are discovered in the process of decision making, and cause management to modify, emasculate and even abandon plans that engineers have recommended as being high in cost efficiency. Management's ineptness in decision making is in large measure responsible for disseminating the idea that cost efficiency is the only goal of investment decisions.

In recent years as more companies have come to consider the possibility of diversifying and expanding, they have been forced to discover the process of making genuine decisions. The annual cost and present worth methods that were customary for replacement problems were bound to be completely inadequate. As knowledge about decision making increased, the customary approaches to replacement were sharply questioned. Why and how is replacement different from expansion and diversification? Or is replacement a special case of a much larger and more inclusive situation - like Ohms law is a special case of the Hertz equations. More and more this special case concept is gaining recognition.

Replacement theory, like Ohms law, is basic and useful. We pay tribute to its value, but unlike Ohms law we have yet to move on. Engineering economy is an anachronism. While close to 50% of industry's annual capital expenditures are for expansion and diversification, engineering economy textbooks and courses concentrate on neat, formalized approaches to replacement. Expansion and diversification are ignored, yet they are the larger, and more perplexing problems that face the engineer. Economics of engineering must be brought closer to reality to be of greater value.

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Regularization of Business Investment, Universities-National Bureau of Economic Research, Princeton University Press, 1954. pp.xxvi, 513. $8.00.

Reviewed by:
Joel B. Dirlam
University of Connecticut

In November, 1951, the Universities-National Bureau Committee for Economic Research sponsored a conference to "explore what individual firms might in their own interest" contribute to high employment levels. Business and academic economists whose papers were collected and edited by M.G. de Chazeau were asked to focus their discussion on regularization of investment. Because of the micro-economic orientation of the conference, which was designed to fill a gap in economic thinking and writing on the business cycle there was, aside from a brief treatment by A.G.Hart, little consideration of domestic governmental stabilization techniques. In fact, the editor went so far as to reject as a possible regularizing device, tax policy, designed solely to stabilize investment. The contributors were, however, chosen from a wide spectrum of opinion, including some, like Walter Hoadley, who were extremely sceptical of, if not actually hostile to, the basic task of the conference. Greater stability could be achieved only by sacrificing "continuing dynamic growth," according to this view.

Cleavages among contributors appeared in other areas, and were so marked as to call for extended comment in each of the able and stimulating appraisals of the work of the conference by Donald Woodward, A.G.Abramson, and Ruth Mack (themselves not wholly in agreement). The 23 authors and commentators took different views of the meaning of regularization, of the length of planning period, of the role of small firm, of the type of cycle that was to be smoothed out. Such diversity is of course not a defect in a symposium of this type; on the contrary it enhances the value of the book. It brings home in an effective, indeed almost frightening fashion, the serious inadequacies in our understanding of the investment process and its variations from firm to firm, and industry to industry, and it outlines the abyss that separates the theory of investment behavior from its actuality.

Joel Dean and de Chazeau, both of whom presented synthetic analyses of individual firm behavior, personify the conflict, perhaps the most important to emerge from the discussion, in theories of investment. Distilling available industry studies and a two-year study of 49 firms, de Chazeau concluded that businessmen evinced a "stubborn resistance" to "'scientific' economic formulas for the timing of capital outlays." He sympathized with the stubbornness because of the unavoidable uncertainty of future flow of net income. Only relatively unimportant investment decisions could be made on a rigorously quantitative basis. The complexity of interractions among the firm's present and anticipated market position, research goals, raw materials control and other factors suffused most investment decisions with the cast of "economic strategy" rather than narrow economic advantage. Dean, on the other hand, presumably relying on his consultative work with large corporations, believed that "the vital practical problem in capital budgeting" was "to obtain reasonably accurate and comparable estimates of return on investment," although he too conceded, indeed (somewhat paradoxically) stressed, forecasting difficulties. As an illustration of budgeting techniques he showed how discounted future returns from the installation of two rival machines could be compared. The core of Dean's investment analysis was the coincidence between a demand curve for capital (simply a schedule of discounted net income from possible projects) and a completely inelastic supply of internally generated funds. He went on to say that a "cut-off" point usually substituted for this supply-demand apparatus.

The factual surveys of investment presented in the book do not, unfortunately, permit a wholly informed choice to be made between rival investment theories. The discussions of the building materials, industrial machinery, petroleum, railroad and electric utility industries ranged widely in their degrees of penetration into the ultimates of investment decision. The electric utility industry apparently comes closest to the application of Dean's investment criteria, because of the ability of the firms to forecast demand. Even here it is hard to understand how

computations of future net returns could be made involving isolated units of equipment like transformers or powerhouses. Kent Healy, in a path-breaking historical review of influences affecting major items of railroad expenditure, gave first place to irrationality and inertia. In a typical decision a very large order "for locomotives was doubled in the course of a few minutes conversation. Expenditures in the other industries seemed to conform fairly closely to de Chazeau's hypothesis that the directors' usual justification for a major expenditure is "needed to meet competition."

Whatever their differences in articulating the measures that businessmen are presumed to use in investing, the contributors agreed that projects were ranked. Given the ranking, volume of spending by the firm is therefore determined by the availability of capital. Firms pick outlets with the fastest pay-outs because of limited funds. Determinants of the flow of funds therefore rise to strategic importance.

Neil Jacoby and Fred Weston, in a meaty and lengthy paper, summarized the published data on corporate financing, drawing mainly on National Bureau sources, and essayed some ingenious estimates, based on Lutz's work and hypothetical dividend pay-outs of the potential level of depression investment. Of necessity, their data are far removed from the individual firm level at which decisions are made. Nevertheless, their approach sheds new light on the nature of capital supply to the individual firm. In prosperity, because top management not only is optimistic, but feels safe in following the crowd, heavy commitments will be made, and funds will be "plentiful." In depression, responsible officials will simply be unwilling to make a conspicuous error. Thus high liquidity will be combined with a shortage of funds. (Admittedly, allowance must be made for shrinkage in earnings.) Moreover, the reluctance of most firms to engage in long-term borrowing, even in booms, ties much of investment to the "cost" of equity capital, which is interpreted subjectively. Some firms apparently are unwilling to float new stock issues unless they can do so at no cost whatever; the paucity of new issues

by corporate giants over the past three decades is notorious. Credit rationing by banks and other financial institutions is reinforced by the spartan self-denial (perhaps caused by reluctance to share control) of firms relying on internally - generated cash. Dean's inelastic supply curve is therefore not an objective limitation to investment, but largely self-inflicted.

Assuming that there is a wide margin for adjustment in the typical investment decision, influenced as it is by rules of thumb and subjectivism in management's estimates of both the number and character of desirable projects and the amount of financial support available, the editor and many of the contributors concluded that some additional regularization of investment was possible. Directors and top executives can be educated to see the futility of heavy commitments at the tail-end of an innovative cycle (an error common among railroads, according to Healy) and to find a place among the "imponderables" responsible for action to benefit the firm and the public by smoothing the investment pattern. Even though, as Hoadley urged, "businessmen should behave like businessmen" so tp behave does not mean eternal reliance on an inflexible and narrowly limited roster of criteria for action.

Apart from its usefulness as a pioneer study of the problem of the private control of regularization of investment, the book should provide an invaluable storehouse of both data and strategic unanswered questions for all those concerned with the nature of decision-making by business firms.

Realistic Depreciation Policy, by George Terborgh, Machinery and Allied Products Institute, 1200--18th St., N.W.,Washington,D.C., 1954. 197 pages,$6.00.

Reviewed by:
Ernest H. Weinwurm
De Paul University

Once again, Dr. Terborgh has called forceful attention to an important problem of industrial management. His new book on depreciation should be carefully studied by everyone concerned with this complex topic; this refers particularly to engineering economists and industrial engineers.

> Terborgh offers by no means a comprehensive analysis of the problem. He is interested in the current methods of computing depreciation which result in charges that are far from adequate to provide funds for the replacement of capital assets. But he does not discuss some of the fundamental aspects of the problem. Readers of the book should be aware of this lack of balance.

Like most other writers on depreciation, he appears to accept the right of the government to interfere with managerial decisions about the appropriate amount of depreciation charges. What he challenges is the inequitability of the governmental regulations rather than the principle as such. Moreover, he looks at depreciation from the standpoint of the investor who wants to have his capital returned. He is not concerned with the adequacy of depreciation charges as part of the current cost of production. As the present writer has explained elsewhere, a satisfactory solution of the industrial aspects of depreciation will have to start from these premises.

Space does not permit a more detailed critical analysis of the book. Therefore, merely a brief summary of its conclusions and recommendations will be given.

While the traditional accounting concept of depreciation rests on the allocation of the original (first) cost of an asset over the period of its useful life, Terborgh emphasizes the value aspect of depreciation. Thus the depreciation allowance is really one for capital consumption. The capital value of an asset is the discounted value or present worth of the sum of its annual service values. The latter are the amount available for capital recovery and profit combined. The pattern of depreciation is the course of the annual service values. But future service values are not known in advance. They have to be estimated.

Service values in general and on the average tend to decline at a fairly constant rate. The probable loss of capital value is concentrated in the early part of the service life. Assuming a ten percent discount rate or rate of return on investment, Terborgh calculates the probable value loss in the first third of life at around 50% for equipment and 45% for buildings and structures while the loss for the first half of the service life is in the vicinity of 70% and 65% respectively. These data are the basis for his "correctly computed depreciation rates" which he defines as accruals that would be derived if the service life of each item were known in advance and each item were depreciated separately on the basis of its own life.

There are three principal reasons for the existing wide gap between these correct rates and those presently computed. The first is the almost universal use of the straight-line method of depreciation with its uniform allocation of original cost over the service life.* The second is the insistence of the Internal Revenue Service on the use of group methods of accounting based on average service lives; this prevents, for all practical purposes, ascertainment of losses from retirement of assets prior to complete liquidation. Last but certainly not least are the effects of the fluctuation of the accounting measure itself (the dollar).

Terborgh uses some ingenious techniques to present quantitative data indicating the magnitude of the gap under different assumptions. This is perhaps the most useful part of his book to substitute data, however rough, for the guesses up to now. The combined effects of the first two causes add up to an accumulated deficit of 30 billion dollars and a current (annual) deficit of 2.6 billion as of 1953. The deficiency from the loss of value of the dollar up to 1953 is 70 billion and the current loss for 1953 amounts to 4.2 billion.

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*This has been modified, however, by the Revenue Act of 1954. (See below)

These are Terborgh's recommendations as released in a special pamphlet prior to the publication of the book into which they have subsequently been incorporated:

1. Authorize the taxpayer to elect a double-rate declining balance write-off. This means an initial rate of $ 200 for every $ 100 of straight line depreciation.

2. Authorize taxpayers to blow-up original-cost book values to their current dollar equivalent based on an official set of multipliers.

3. Let the first recommendation have precedence over the second and have it spread over no more than a few years.

A small part of these recommendations was actually incorporated into the Revenue Act of 1954. Use of the declining balance method at double rate was authorized but only for assets acquired after January 1, 1954. Thus, there has been no making-up of the accumulated deficit and that still to be accumulated by assets acquired prior to January 1, 1954. On the other hand, the 1954 Act authorized the use of the sum-of-the-digits method which provides even larger depreciation over the early years of service life than the declining balance method. In addition, a change-over from the declining balance method to the straight line method was permitted to eliminate the undepreciated balance arising under the declining balance method.

Evaluating Intangibles for Executive Decision, by A.L.Stanly, Director of Planning, Associated Missile Product Corporation, Pomona, California. Mechanical Engineering, September, 1955.

Reviewed by G.W. Mc Elrath, University of Minnesota
J.E.Bearman, University of Minnesota

Following is a brief outline of Mr. Stanly's approach:

Executive decisions involve the exercise of judgment and intuition, "the spontaneous integrated result of past experience, logical reasoning, and personal preference" because of the intangible nature of factors that cannot be readily evaluated quantitatively. Mr. Stanly's aim is to bring some of these factors into tangible form and make them susceptible to numerical treatment. He uses the statistical concept of "Expected Value" or "Mathematical Expectation" to present his ideas, and presents two problems which illustrate his method. They are " a simplified but otherwise indicative problem" and a "sample complex problem."

Mr. Stanly's first problem concerns a factory owner who wishes to expand his facilities and -- other things being equal -- prefers to locate in California. However, if an earthquake strikes his ONE factory he suffers damages to the extent of $100,000. Further, he has valid reasons to estimate that the risk of such an earthquake's striking within the next 10 years is approximately 1/2000. Hence, the expected cost of an earthquake within the next 10 years is

$$\$100{,}000 \times 1/2000 = \$50.$$

He concludes that, "Even if it (the expected cost, $50) is in error by a factor of 10,. . . the expected cost of earthquakes is relatively very small. Hence our manufacturer would eliminate earthquake damage as a serious consideration, and would make his decision on the basis of the remaining factors."

Stanly's second problem, the remainder of the paper, is concerned with a five component missile assembly. He wishes to compare the relative merits of field assembly versus depot assembly. The following intangibles must be evaluated:

(1) Logistics (with respect to supply effectiveness);
(2) Reliability (of missiles);
(3) Explosive hazard.

He proceeds to obtain expected costs in each of these three areas for each of the alternatives of field assembly and depot assembly. In addition, he obtains estimated maxima and minima around these expected costs. The results are summarized in a table yielding total expected costs with its associated maximum and minimum costs for each of the alternatives. A decision is made on the basis of the comparison between the differential costs under the two alternatives. He concludes ".....the proposed technique of evaluating intangibles should always be at least a better guide than simple intuition since it requires a breakdown and examination of the contributing factors of each consideration and thereby localizes and minimizes the possibility of error."

Comments by Messrs. Mc Elrath and Bearman:

The concept of "Expected Costs" or "Expected Value" is a valid statistical notion which can serve as a powerful tool to help analyze situations involving incomplete information. The ingredients of expected cost include repeated situ- in each of which the same event may or may not occur; the cost to a concern each time the event happens; and the probability of the occurrence of the event. In the simplified situation in which the probability remains constant, the "Expected Cost" is obtained by forming the product.

Expected Cost = Number of situations x cost (if the event occurs)
x probability that the event will occur.

Note that the "Expected Cost" concept is validly applied when there are many situations of like nature over which we can balance our losses against favorable outcomes. This allows the "law of averages" to take hold and operate as it must if the notions of probability and expectation are to be correctly applied.

Consider the following modification of Mr. Stanly's problem, in order to illustrate the concept of "Expected Cost." Suppose Mr. Stanly's manufacturer, who is anxious to expand his facilities, decides to build N new plants, call them $P_1, P_2, \ldots . P_N$. They will be located at N scattered points throughout the world so that any single earthquake that may occur will damage at most one plant. Let the earthquake damages be, respectively, D_1 for P_1 , D_2 for P_2,..., and D_N for P_N. Further, let the probability of occurrence of earthquakes at the respective locations over, say, the next ten years be estimated as p_1 for P_1; p_2 for P_2, . . . , p_N for P_N . Now the "Expected Cost" of earthquake damage to the overall manufacturing facility over the next ten years is calculated as follows:

$$\text{"Expected Cost"} = D_1 \times p_1 + D_2 \times p_2 + \ldots + D_N \times p_N$$

$$= \Sigma D_i \times p_i$$

where each $D_i \times p_i$ is the equitable sum paid into the common fund by factory i to cover the loss that might be incurred by some unknown factory. Note that when all D_i are equal and all p_i are equal, this situation reduces to the "simplified situation" above. One might conclude that with N = 1 the "Expected Cost" is merely $D_1 \times p_1$; and formula-wise this is exactly the case. However, $D_1 \times p_1$ is not the cost to our manufacturer; if there is no earthquate, the cost is zero, and if the earthquake does strike he is wiped out to the extent of $100,000.

The difference between the ONE plant situation and the MANY plant situation is clear. The occurrence of earthquake damage at any one of the many plants merely inconveniences the total operation. The sum of $D_i \times p_i$ from each and every one of the plants adds up to the total expected cost, i.e., the cost of repairing the damage. Notice that "Expected Costs," with minor modifications, is the basic idea of casualty insurance.

Mr. Stanly correctly uses the concept of "Expected Costs" in his "sample complex problem." It is very understandable - as Mr. Stanly suggests - that first attempts to estimate the intangibles have small chance of being optimum. For example, the estimations of "probability of loss" in Stanly's complex problem will doubtless be better on next consideration after gathering more data.

(What follows must not be construed as criticisms of the principles involved in the paper; rather, it is suggested to help refine the estimations on further consideration of the problem.) First of all, Stanly assumes that missile reliability is 100% in the case of depot assembly. If so, those of us in Quality Control can either go out of business or else take a well deserved bow. We have never before been able to guarantee 0 percent defective — particularly when the only test of the product is a destructive test, as in the case of an explosive. Second, in arriving at the expected cost of explosive hazard for depot-assembly, he considers only the cost of missiles in a trainload, the cost of the freight train, and the loss in lives, valued at $50,000 per life. Admitting his figures, he concludes: "Thus the total expected cost of an explosion is estimated to be of the order of $50,000,000." Inasmuch as the most likely time for an explosion of a missile train

would be in the process of switching with its attendant jarring and jolting, the most likely place would be in large freight yards and hence in or near large metropolitan areas. Need one go into detail to have Stanly's figure revised in favor of a figure ten, a hundred, or a thousand times as large, when one considers the destruction to be wrought by 100 missiles demolishing the environs of the freight yards of any one of our major cities?

As another instance, Stanly says,"About the best that can be said is that the proportion of missiles that fail because of field, instead of depot assembly of explosives will be small -----greater than 0 percent and probably (our italics) less than 5 percent, with an average estimate of, say, 2.5 percent." Even were the proportions between 0 and 5% -----and Stanly isn't certain of that ----- there is no way of knowing that the average estimate is anything, let alone 2.5 percent, unless we know the probability distribution of the proportions. True, Mr. Stanly does make allowances for the unknown elements by setting up estimated ranges around his expected costs. However, his expected costs and ranges are subject to re-evaluation in the same spirit, since they seem to be only educated guesses.

These are examples of judgment estimates that Stanly's efforts are starting to quantify. By letting Mr. Stanly's concepts and thinking infiltrate the company's decision making policies, Associated Missile Product Corporation will benefit financially. The father any company will allow such concepts to permeate its management policy making, the better off that company will be.

> This paper should be valued for its attempt to bring a measure of precision into an area where "by-heck-and-by-gosh" have heretofore been the only method used. Stanly's method is,perhaps without his being aware of it, that of trying to find a mini-max solution. He is trying to find that situation which minimizes the maximum loss among all possible outcomes. (It would be well for more people in endeavors such as this one to familiarize themselves at least with the elements of the Theory of Games*). Stanly is moving in the proper direction; and while these are perhaps only the first steps,later ones will be easier and surer, owing to those already taken by Stanly as he so aptly concludes in his paper,"The proposed technique of evaluating intangibles should always be at least a better guide than simple intuition since it requires a breakdown and examination of the contributing factors of each consideration and thereby localizes and minimizes the possibility of error."

*Von Neuman & Morgenstern; Mc Kenzie, to cite two sources.